of our solar system which cannot be self-generated, the possibility of having evidence of anti-matter in comet tails and coronal streamers, and the idea of potential matter.

ARTHUR SCHUSTER.

Solar Radiation.

AT the conclusion of his British Association lecture on Phosphorescence,[1] Mr. Jackson makes a suggestion with regard to solar radiation which will doubtless receive due attention from those who are interested in solar physics. It is one of especial interest to me because, by an entirely different train of thought, Mr. Jackson has arrived at a possible explanation of the relation between sun-spots and terrestrial magnetic disturbances which is practically identical with a suggestion I have recently put forward in a paper on "The cause of the darkness of sun-spots," published in the *Astrophysical Journal* (April 1897).

In this paper I attempted to show that absorption by relatively cool material offers no satisfactory explanation of the darkness of sun-spots, and that the spectroscopic evidence is really quite compatible with a relatively high temperature even in the umbra of a spot.

But in abandoning the absorption hypothesis, one is brought face to face with an apparent contradiction of Kirchhoff's law. Thus it is certain from the low mean density of the sun that the interior region under enormous pressures must be vastly hotter than the photosphere. If, therefore, spots are really breaks in the photospheric clouds through which we obtain a glimpse of the interior, why is it that the radiation from them is apparently so much less intense than from the photosphere? The clouds of condensed matter may, of course, possess a much higher radiating power than the gaseous mass below them; but this, according to Kirchhoff's law, should be entirely compensated by the enormous depth of the feebly radiating interior mass.

To meet this difficulty I suggested that the radiation from the interior, at the transcendent temperatures which must exist even a few thousand miles below the sun's visible surface, may possibly not be apparent as visible light, but may occur in wave-frequencies of a higher order than the known spectrum; and "may be effective in producing those magnetic disturbances which are characteristic of large umbræ."

Mr. Jackson however, if I have rightly understood him, supposes that it is not so much a question of temperature as of molecular structure that determines the wave-frequency of the radiation; and he regards the light of the photospheric clouds as a phosphorescent glow induced by undulations of a high order of frequency which are emitted by the simpler uncondensed materials. The condensed clouds containing more complex molecular groups acting as a screen, and converting the invisible radiant energy of high frequency into ordinary light.

With regard to this interesting speculation, one would like to know more particularly what is the nature of the evidence on which the idea is based that very simple molecular systems give rise to undulations of high frequency? There can scarcely be any analogy between the behaviour of matter in highly exhausted tubes and under the enormous pressures and temperatures which must exist within the photosphere.

The case of the phosphorescent limes is an exceedingly interesting one; but is there any ground for the belief that the lime obtained from organic salts, and giving a blue phosphorescence, is really simpler in molecular structure than a lime which glows red?

J. EVERSHED.

Kenley, Surrey, October 14.

Hibernating Reptilian Embryos.

WILL you allow me space to correct an error that has crept into the account given in the *Christchurch Press*, and reprinted in the last number of NATURE (p. 609), of Prof. Dendy's successful investigation of the development of the egg of the Tuatara lizard, *Sphenodon*.

The fact of an embryo hibernating within the egg was not, as stated, unknown among vertebrates, an exact parallel being offered by no less well-known a reptile than the European pond-tortoise (*Emys orbicularis*). This was first observed in Austria, in the last century, by Marsigli, whose statement has been corroborated by Miram in 1857, eggs laid in his garden at Kieff in May hatching eleven months later, and by Rollinat in 1894, the latter author concluding that hatching does not, as a rule, take place in France before the twenty-second or twenty-third month after oviposition.

I need hardly add how pleased I feel at the result of Prof. Dendy's investigations showing the close resemblance which the development of *Sphenodon* bears to that of the tortoises, since I believe to have been the first systematist to follows Cope (1885) in placing the Rhynchocephalia in close proximity to the Chelonia with the remark: "The affinities of the Rhynchocephalia to the Chelonia are at least as great as to the Lacertilia" (Cat. Chelon., 1889, p. 1).

G. A. BOULENGER.

British Museum (Natural History), October 23.

Organic Variations and their Interpretation.

I SHOULD be glad if Mr. Cunningham would tell us upon what evidence he founds his opinion that, in crabs, "it is certain that the number of ecdyses depend on age, not on size."

This assumption lies at the base of Mr. Cunningham's criticism of Prof. Weldon's arguments; but, even apart from that, the matter is one of such general biological interest that I hope he will respond to an invitation to substantiate a view which to me, at any rate, is altogether novel. I have always understood that exuviation was a phenomenon essentially connected with the process of growth in Crustacea rather than with the mere passage of time, and it is needless for me to remind Mr. Cunningham of the familiar facts and published statements which support this generally accepted view. Will Mr. Cunningham, on the other hand, tell us how many cases of exuviation, unaccompanied by growth, he has observed among Crustacea?

Unless Mr. Cunningham can revolutionise the present state of knowledge on this subject, his criticism, based on the greater relative growth of young crabs in 1893 than in 1895 and 1898 (which in itself is probable enough), falls to the ground; for he admits that "change in the proportions of a crab occurs only at the ecdysis." In assuming that, on the whole, similarity of size in young shore-crabs indicates an equal number of moults, Prof. Weldon appears to me to be quite in accord with our present knowledge of the subject. Certainly—to modify Mr. Cunningham's phrase—the frequency of exuviation in different *Carcini* corresponds much more closely with their relative growth than with the periods of time occupied.

Plymouth, October 22. WALTER GARSTANG.

Wall Mirages.

MR. R. W. WOOD, who describes a mirage on city pavements, in NATURE of October 20 (p. 596), may like to refer to the second volume of NATURE (p. 337, August 25, 1870), where he will find an account of mirages seen by looking closely along a wall, which was exposed to a hot afternoon sun. The mirage must be very common, and needs only looking for. Mr. Wood's interesting letter may lead others to photograph this curious phenomenon in our own country. A wall will be easier to deal with than a pavement.

C. T. WHITMELL.

Leeds, October 22.

A White Sea.

I HAVE received several letters respecting this phenomenon (see p. 496), and have distributed the samples of water to two gentlemen who were desirous of examining it. Will you permit me to say that subsequently I received another application from a bacteriologist on the continent, and that the letter was unfortunately lost before complete perusal. Hence my failure to acknowledge its receipt.

JAMES W. BARRETT.

22 Cavendish Square.

SURFUSION IN METALS AND ALLOYS.[1]

THE author points out that metals and alloys may be maintained in a fluid state at temperatures which are many degrees below their true freezing points, and states that this fact has been but little studied. As regards salts, the question of surfusion has recently received much attention. Ostwald (*Zeit. für Physikal. Chem.*, 1897, vol. xxii. p. 3) has shown, as the result of an investigation of great interest, that a very minute quantity of a solid will cause a mass of the same substance to pass from the surfused to the solid state. His work, moreover, has led him to distinguish between the *meta-stable*, or ordinary condition in which surfusion takes place, and the *labile* condition which occurs at temperatures much below the melting point. Ostwald's paper, and one by M. Brillouin (*Ann. de Chim. et de Phys.*, 1898, vol. xiii. p. 264), on the theory of complete and pasty fusion, led the author to offer the Royal Society the results of his own experiments on the surfusion of metals.

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[1] NATURE, October 6, p. 562.

[1] "Surfusion in Metals and Alloys." By Prof. W. C. Roberts-Austen, C.B., F.R.S. (Abstract of a paper read at the Royal Society, May 26.

Metals do not appear to have been examined from the point of view of surfusion until the year 1880, when some excellent experiments on the surfusion of gold were made by the late Dr. A. D. van Riemsdijk (*Ann. de Chim. et de Phys.*, 1880, vol. xx. p. 66), by whose early death, which occurred last year, Holland has lost a skilful physicist. He pointed out that:—

"Faraday, in his memoir on regelation, published in 1858, stated that acetic acid, sulphur, phosphorus, many metals and many solutions, may be cooled below the freezing temperature prior to solidification of the first portions" ("Experimental Researches in Chemistry and Physics," p. 379). On the other hand, in their treatises on physics, Danguin (vol. i., 1855, p. 892) and Jamin (vol. i., 1859, p. 105) mention tin as the only metal which is capable of remaining liquid at a temperature 2·5° below the true solidifying point of the metal.

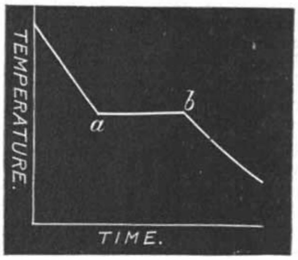


FIG. 1.

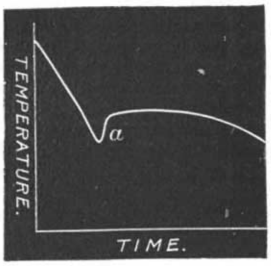


FIG. 2.

Van Riemsdijk's contribution to the subject of surfusion of metals consisted in showing that the well-known phenomenon of *éclair*, the brilliant flash of light which often attends the solidification of the metal in the ordinary assay of gold, is really due to surfusion. He also pointed out that surfusion could be either stimulated or hindered by suitably modifying the conditions, but he made no attempt at thermal measurements. It was not until ten years after van Riemsdijk's work that the recording pyrometer, which the author submitted to the Royal Society in 1891 (*Proc. Roy. Soc.*, 1891, vol. xlix. p. 347), enabled such measurements to be readily effected.

After a brief description of this appliance, the nature of which is now well known, it is stated that the freezing point of a metal, or the initial freezing point of an alloy, may be represented by one or other of three typical curves. Two of these are shown in the accompanying figures, which indicate the nature of the curves, traced by the recording pyrometer. Fig. 1 shows the freezing point curve of a pure metal, the horizontal portion, *a b*, indicating the actual solidification of the mass, the sharpness of the angles at *a* and *b* attesting the purity of the metal. The initial freezing point of most alloys would resemble Fig. 1 in having the corner *a* sharp, while the point *b* is generally rounded off.

The third type of curve, which may be a modification of the other two types, indicates the occurrence of surfusion, the bend at *a*, Fig. 2, showing the amount of surfusion which was observed. The author has detected pronounced cases of surfusion not only in gold, but in copper, bismuth, antimony, lead, and tin. Surfusion, moreover, is not confined to pure metals, and he showed in 1893, that the eutectic alloy in the bismuth-copper series presents a marked case of surfusion. In order to study surfusion, it is necessary to make the galvanometer (to which the thermo-junction is attached) very sensitive. The method of effecting this is described, the thermo-junction itself being in all cases suitably protected and placed in the cooling mass of metal or alloy. A curve, traced by the aid of such a sensitive method, if it represents the surfusion of a metal or an alloy, does not merely show a slight depression as in the case of pure gold shown at *a*, Fig. 2: the slight depression becomes a deep dip. It is, in fact, possible by the methods described by the author to ascertain what takes place during the surfusion of an alloy, and the results are shown in two plates appended to the paper. From these plates one illustration (Fig. 3) has been selected. It is the autographic representation of the surfusion of an alloy of 64 parts of tin and 36 parts of lead. The line *a b* represents the surfusion of the mass which, as the scale shows, fell 10 degrees below its true point of solidification before it actually became solid. The solidification of the mass is recorded by the horizontal line *c*. This autographic record also shows that something happened during surfusion, for there are points at *d* and *e*. These proved to be due to the falling out of lead at *d*, and to its having to be remelted at *e*. The entire mass then became solid.

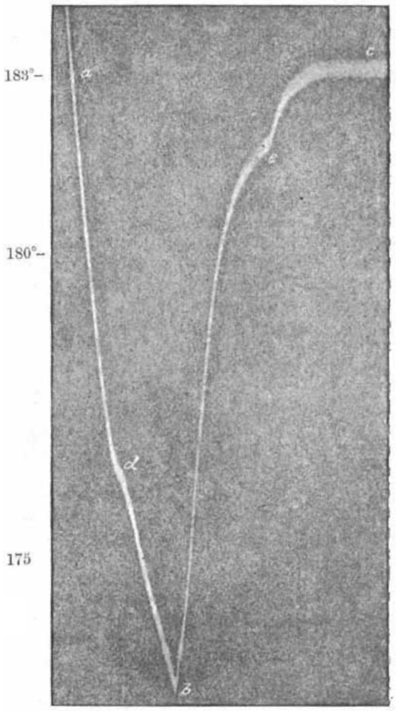


FIG. 3.—64 tin, 36 lead.

Experiments such as the one described have enabled the author to trace the crossing of solubility curves of certain metals in each other in the same way as had previously been effected in the case of salts by H. le Chatelier and by Dahms.

This crossing of the solubility curve of lead and tin is shown in Fig. 4, but for a description of it reference must be made to the original paper.

The first experimental evidence as to the identity of the behaviour of saline solutions and metallic alloys as regards selective surfusion, has thus been afforded by

Prof. Roberts-Austen. The question is, as he shows, one of much theoretical interest, and should lead to further experiments.

The author then adopts a method previously used by Spring (*Bull.* Acad. Roy. Belg., vol. xxviii., 1894, p. 40). He proceeds, after quoting experiments by Ostwald, Demarçay, Pellat, Colson and Russell, to show that alloys may be formed by the vaporisation of certain metals *in vacuo* at so low a temperature as 50° C. He concludes by pointing out that the results given in the present paper reveal additional points of similarity between the behaviour of alloys and that of ordinary saline solutions. He trusts, therefore, that it may be useful as a continuation of his investigation on the "Diffusion of Metals," which formed the subject of the Bakerian Lecture of 1896.

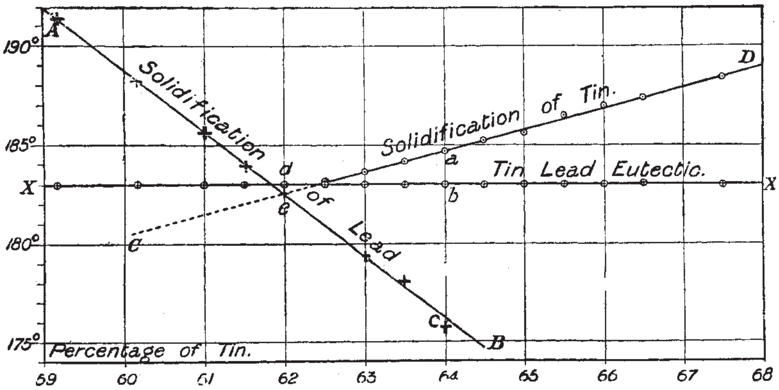
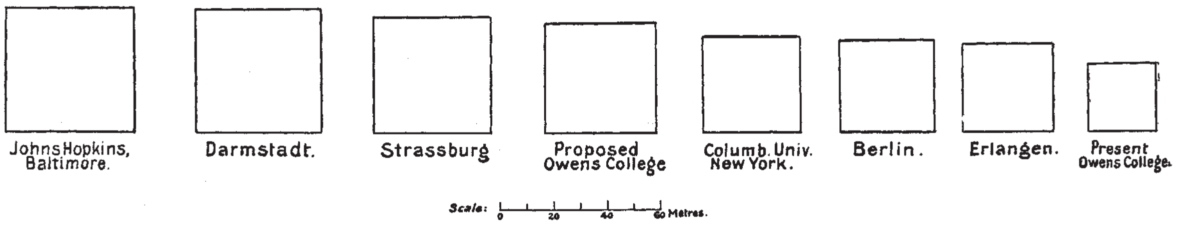


FIG. 4.—Freezing point curves of lead tin alloys.

THE NEW PHYSICAL LABORATORY OF THE OWENS COLLEGE, MANCHESTER.

THE laboratory of which the foundation-stone has been laid, on the twenty-fifth anniversary of the occupation of the present Owens College buildings, will be the largest and most completely equipped in this country. It stands on a separate plot of ground adjoining the Owens College site, and consists of a main building and a large annexe, the latter being more especially intended for electro-technical work. The principal building is 100 feet long and over 60 wide, and consists of a basement and three stories. The diagram gives the comparison as regards dimensions with some of the principal laboratories abroad. The squares represent square area of floor space of the working rooms, *i.e.* all corridors, cloak-rooms, &c., are excluded, and the floor space of the different stories added up. It will be seen that the only laboratories materially larger than the proposed building are those of Baltimore and Darmstadt. But provision has been made for future extension, the plot of ground secured by the College being sufficiently large to double, if necessary, the size of the building.



The cost of the building with fittings and new apparatus is estimated at 30,000*l.* Of this sum 17,000*l.* has been subscribed, in one sum of 10,000*l.*, one of 5000*l.*, and two of 1000*l.* In the plan ample provision for research work has been made. Two large rooms, for instance, are exclusively devoted to spectroscopic work, one of them being arranged to hold a large Rowland grating. It is intended to have at least one room set aside for constant temperature work, and to establish a small plant for the production of low temperatures. An electro-technical laboratory will be added, in which large currents will be available for electric furnaces.

One of the features of the laboratory will be a carefully planned system of ventilation combined with an attempt to exclude dust, as far as possible, from all rooms, and especially from the instrument cases. The plenum system, much used at present, had to be rejected, because it takes up too much valuable basement space, because it is ineffective as regards exclusion of dust, and because the inevitable noise and mechanical shaking due to the fans would have seriously interfered with the work of the laboratory. The architect is Mr. J. W. Beaumont, who, before finally drawing the plans, was sent by the Council of the Owens College to visit the principal modern laboratories of Germany.

In seconding a vote of thanks to Mr. Henry Simon for laying the foundation-stone, Prof. Schuster gave a short description of the building. In the course of his remarks he said :—

> In the general plan of the building I have departed considerably from that adopted in some of the recent continental buildings. The designer of a laboratory may take either one or other of two opposite views, according as he wishes to differentiate as much or as little as possible between different rooms and between different classes of students. The present tendency is to adopt the former course, and to draw a rigid line of separation between the rooms set aside for elementary and for advanced work. This system is carried out to such an extreme in one of the most recent and, in some respects, most perfect of German laboratories that a separate division with a staircase of its own is provided for the elementary students, who thus can never be brought into contact with their more fortunate colleagues admitted to the main part of the building.
>
> I have adopted the opposite course, for I consider that a free intercourse between different classes of students is of great benefit and educational value. My object has been to throw the students together and not to separate them, so that the beginner may occasionally see his more advanced colleagues at work, and the latter will have an opportunity to overlook and sometimes to assist their juniors. To some extent, the separation of students is necessitated by the requirements of space and apparatus, but we may do much to minimise instead of exaggerating the division.
>
> Most of the rooms devoted to the highest kind of work are sufficiently large to accommodate several students. In this matter also I have not followed the practice now in fashion, which favours small rooms for single students. It is no doubt